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The Quality of Data Reported on Birth Certificates

The publication of the paper by Watkins et al. on the utility of the revised birth certificate for surveillance of birth defects could not be more timely.[1] Although the "accuracy" and "completeness" of reporting of clinical data on birth and fetal death certificates have been assessed before,[2–4] rarely are these data held to the more scientific standards of validity and reliability.[5,6] Although no previous validation studies focused on reported congenital anomalies in the peer-reviewed literature, many public health professionals and advocates continue to believe that birth defects can be monitored with the use of birth certificate data. For example, a case–control study of gestational diabetes as a risk factor for birth defects used congenital anomalies reported on birth certificates as the outcome measure.[7] More instructive is an analysis that documents the limited contribution of birth certificates in the context of a multisource birth defects registry.[8]

Watkins et al. review several factors responsible for the poor showing of birth defect reporting on vital records. Although one might expect that we could anticipate a gradual improvement as the new birth certificate forms become more familiar and as electronic birth certificate reporting systems proliferate, the situation has instead probably worsened in recent years. Electronic birth certificate reporting has some paradoxical effects where data quality are concerned. Because numerous edit checks are now applied to birth certificate data before these are filed by the birthing hospital, state vital statistics offices provide considerably less scrutiny of the clinical data than they formerly did. These agencies have suffered draconian budget cuts in recent years and no longer have the staff necessary to implement and maintain programs to continuously improve data quality. Also, decreasing newborn lengths of stay will lead to the diagnosis of fewer birth defects prior to the filing of the birth certificate, further reducing the probability that a potentially identifiable birth defect present at birth will be reported on the birth certificate. As a case in point, in the state of Wisconsin, birth defects were reported for 16.9/1000 live births in 1988 on the old certificate form, and this increased to 24.2/1000 in 1989 on the revised certificate form. The proportion of live-born infants with congenital anomalies reported on their birth certificates decreased in each subsequent year; by 1994, only 12.4/1000 live-born infants had birth defects reported, a proportion lower than in any of the 6 years preceding the implementation of the checkbox question.[9] The findings reported by Watkins et al. may actually represent a best-case scenario, given the extensive interaction between Centers for Disease Control and Prevention (CDC) staff and both the Georgia Department of Human Resources and the various hospitals and health care facilities routinely visited by Metropolitan Atlanta Congenital Defects Program staff.

Data quality should be a paramount concern for any state or national agency responsible for the collection and analysis of population-based health data. Until the reliability and validity of natality data collected through the checkbox format has been demonstrated, I propose that this and other peer-reviewed journals impose a moratorium on the publication of statistical analyses based on these data. □

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Cultural Orientation: An Individual- or Group-Level Variable?

In his editorial, "Paradox as Paradigm—The Health Outcomes of Mexican Americans,"[1] Scribner draws a distinction between group and individual variables that appears unjustified. For instance, he states at the outset that "Hispanic health [as exemplified by favorable birth outcomes] represents a group-level correlation between ethnicity and mortality that cannot be explained in terms of an individual-level model." But

later he explains that "Mexican-Americans as a group smoke less, drink less, and eat a better diet than do non-Hispanic Whites." Presumably this means that they also have these favorable attributes as individuals. The distinction that he sets up between group-level variables and individual-level variables is thus overdrawn. There is no essential difference in causal logic between the notion that cultural background might affect birth outcomes through effects on smoking, drinking, and diet and the notion that obesity might affect coronary disease through effects on blood pressure, serum high-density lipoprotein cholesterol, and glucose intolerance. Cultural orientation can be measured and studied as a risk factor, just as can obesity. I see no conflict here with the utility of the "reductionist paradigm" that has been central to advances in causal understanding in both public health and medicine.

That life-style factors may be more important determinants of health outcomes than medical care (including prenatal care) should not be news to public health professionals. Scribner is probably correct in his assertion that the value of prenatal care has been overstated. However, this is not a good example of the ecologic fallacy. There have been numerous studies at the individual level showing that women who receive prenatal care have better outcomes than those who do not; the problem is that most of them are probably confounded by life-style factors.

Scribner's assertion that a "group level model of risk . . . has been virtually ignored by the research establishment" is, perhaps, in the eye of the beholder. Public health professionals interested in preventing cardiovascular disease and cancer have been arguing for decades that it is important to reduce smoking and saturated fat intake in all Americans, not just those at high risk. These strategies have had considerable success in changing societal norms, thus curtailing the epidemic of smoking among men and achieving major reductions in dietary fat intake and coronary disease mortality.

I believe we can all agree that future group interventions hold great promise for further improvements in the public health. □

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Scribner Responds

Public health is a pragmatic science in the Deweyian sense that the truth of a paradigm for public health depends on its usefulness in making sense of observations and guiding action. Rhoads correctly contends that "cultural orientation can be measured and studied as a [individual-level, biomedical] risk factor"; however, pragmatism demands that the usefulness of doing so should determine whether or not the individual-level paradigm of biomedicine or the group-level paradigm of public health represents the best approach for making sense of our observations and guiding future action.

Characterizing cultural orientation as a life-style factor, as Rhoads suggests, reduces acculturation to the causal logic of biomedicine. The causal logic of biomedicine assumes that life-style factors are the result of individual life-style choices governed by interior forces. However, behavior influenced by one's cultural orientation is not governed by interior forces but by exterior forces that shape the behavioral norms and values defining one's culture. Thus, characterizing cultural orientation as an individual-level variable makes it impossible to understand why a Mexican cultural orientation is protective and why the health behavior of millions of Mexican Americans changes for the worse as they passively adopt US culture.

Rhoads' assertion that addressing group-level risk has been a common strategy used in public health is only half true. While chronic disease epidemiology has successfully used group-level methodologies to identify group-level risk factors, it is common practice in public health to address these factors at the individual level. The dominance of the biomedical paradigm causes this shifting of focus from the group level, where risk factors are identified, to the individual level, where causal models are conceived and intervention strategies are devised.

The controversy over the effectiveness of prenatal care is an example of this practice. In the absence of prospective studies using random assignment to control for individual-level confounders, group-level associations are presumed to demonstrate that prenatal care is an effective individual-level prevention strategy. This assumption is ecologically fallacious.

I believe the widespread practice in public health of shifting the focus from the group level, where risk factors are identified, to the individual level, where causal models are conceived, is responsible for the failure of chronic disease epidemiology to translate success in identifying group-level risk to success in conceiving prevention strategies. It also appears to be the basis of Rhoads' confusion regarding the role of group-level analysis as it relates to the paradox of Hispanic health and his inability to comprehend the inadequacy of the biomedical paradigm for addressing group-level risk. □

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